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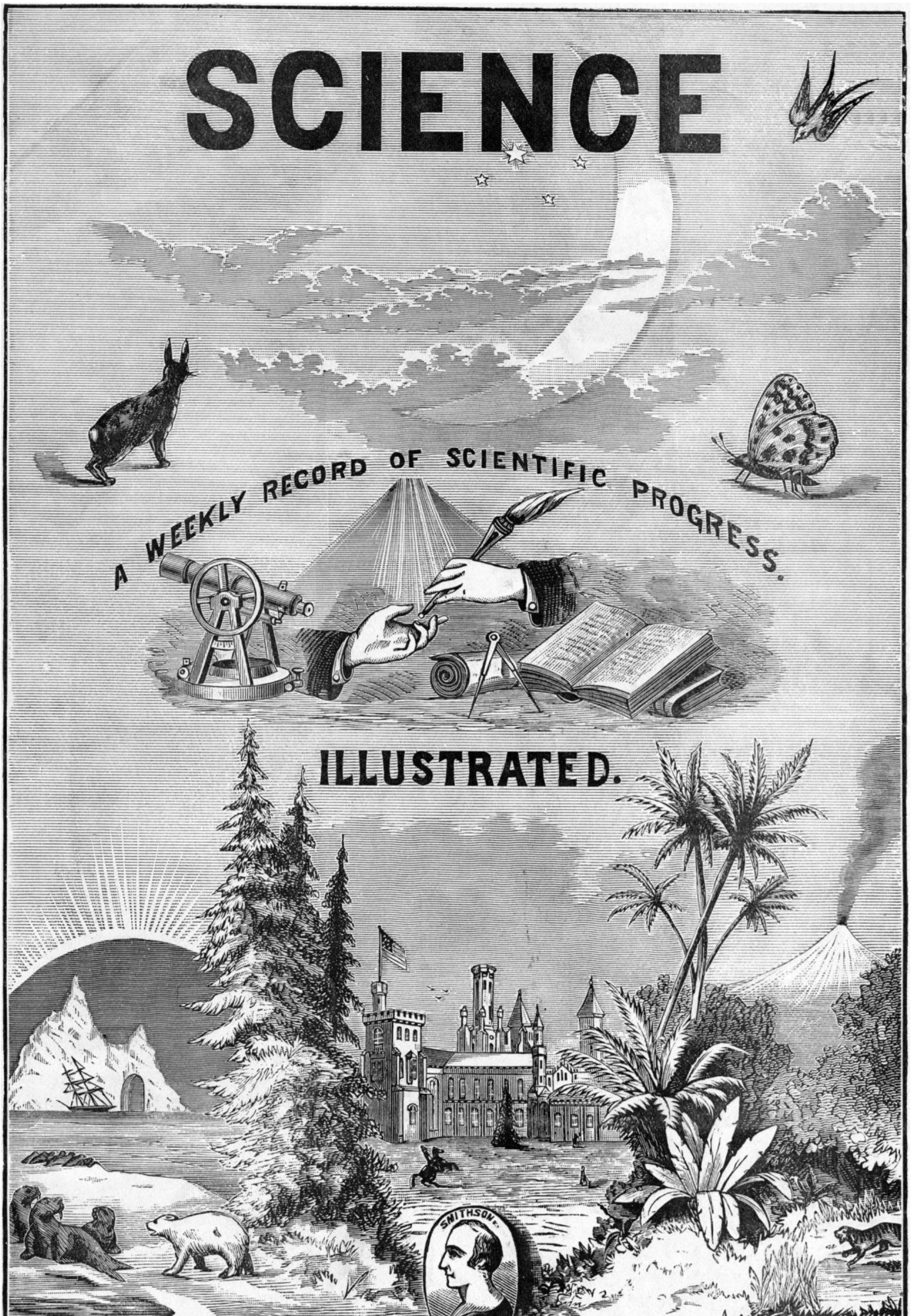

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"SCIENCE—A Weekly Record of Scientific Progress." It is a handsome, well-printed quarto sheet of 12 pages, with 4 addditional pages for advertisements serving as a cover. It counts among its list of contributors Professors Baird and Coues, of the Smithsonian Institution; Holden, of the Naval Observatory, Washington; Marsh, of Yale; Wilder, of Cornell; Young, of Princeton; Abbe, of the Signal Office; Morse, of Salem; Drs. Hammond and Spitzka, of New York, and a considerable number of well-known scientific experts. IT CLAIMS, WITH TRUTH, TO OCCUPY A HITHERTO UNOCCUPIED FIELD IN AMERICAN PERIODICAL LITERATURE, AS THE ONLY FIRST-CLASS WEEKLY JOURNAL IN THE UNITED STATES DEVOTED TO SCIENCE.

SCIENCE

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

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A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

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SATURDAY, JULY 9, 1881.

OBSERVATIONS UPON THE COMET AT PRINCETON.

The comet has been seen and observed every night since June 25, except on June 30th. Every night, however, except July 2d and 3d, the observations have been interfered with by clouds, so that very little continuous thoroughly satisfactory work has been possible.

The light has fallen off rapidly. On the 26th, the comet was for half an hour better seen than at any other time, and the nucleus was judged to be just about equal to Arcturus in brilliancy. On July 2d, it was compared pretty carefully with the Pole Star and with α Urs. Majoris by *squinting*, so that the blurred images of star and comet were brought close alongside. I judged it just equal to Polaris and about $\frac{1}{4}$ to $\frac{1}{3}$ of a magnitude fainter than Dubhe.

The nucleus and coma have presented a very interesting series of telescopic phenomena, in the main such as have been seen in all other large comets. It is noteworthy, however, that immediately behind the nucleus no strongly marked dark shadow-like stripe has been developed, nor, what is perhaps just as common on the contrary, any bright central streamer. On the whole, the central portion of the tail has been a little less brilliant than the edges, even close to the head, but the difference has been slight. On the 25th, the nucleus about 10 P. M. showed 5 projecting jets, much like the pseudopodia of some low animal organism—not well formed, nor distinct, nor symmetrical,—their length from two to six times the diameter of the nucleus, those on the front of the nucleus being the longer.

On the 26th, the nucleus was almost entirely surrounded with a nearly complete, well defined, circular envelope about 1′ in diameter. In this envelope was a curious oval vacuole, behind the nucleus, but on the preceding side of the axis of the tail.

On subsequent evenings no envelopes nearly so complete were noted—only jets of varying length and position, those on the side of the sun being apparently blown back, like flowing hair, by some solar repulsion.

On the 29th there was but one jet on the sunward side, and this was curiously curved toward the preceding side, making the whole look like a comma. (We use preceding rather than Western, because below the the pole where the comet was, the terms Eastern and Western might lead to misapprehension.)

On July 1st the head was curiously unsymmetrical. The coma was extended out in the South following direction like a great liberty cap, the axis of the principal jet which divided both ways, in front like hair parted in the middle, being inclined some 50° to the line of this extension.

With the spectroscope a number of observations have been made.

The nucleus has generally given a simply continuous spectrum, extending from below C well above G; but on June 25th and July 1st, it showed distinct banding at points where the bands of the spectrum of the coma crossed it.

This was seen by several observers on the 25th, and by both Mr. McNeill and myself on the 1st.

The spectra of some of the brighter jets had been caught and isolated several times. They were in all cases continuous, without detectable bands of any kind.

The spectrum of the tail was found to be continuous, with a faint superposed band-spectrum, the same as that of the coma. On July 1st and 2d this band-spectrum was distinctly traceable to at least 15′ distance from the head of the comet, the continuous spectrum perhaps 5′ or 10′ further.

The spectrum of the coma consisted of the usual three bands; but both the upper and lower bands, though pretty bright, were very ill defined; so much so, that I could obtain no satisfactory measurements of wave length, farther than to observe on June 25th and 26th, that the lower edges of the upper and lower bands of the so-called 'first' spectrum of Carbon, (λ, 5635 and 4740) given by a Bunsen burner, fell apparently near the lower limit of these two bands in the comet spectrum as seen with a one prism spectrum. But these comet-bands did not look at all like the flame bands, the difference of appearance being so great, as somewhat to shake my belief for the time being, in the identity of the two spectra.

The middle band, on the contrary, was perfectly defined at its lower edge, and with the one prism spectroscope distinctly showed three fine lines in the band, and these, so far as could be judged, coincided exactly

with the three lines in the middle band of the carbon spectrum.

This resolution into lines was seen by Professor Brackett, as well as by Mr. McNeill and myself, on June 29th; it was still evident on July 2d, but no longer on July 3d.

The coincidence with the middle band of the flame spectrum, has always appeared to be precise; but to obtain further evidence as to the exact position of the band a careful comparison was made on July 2 with the *b* lines of the magnesium spectrum, using the Grubb spectroscope with a dispersive power of *four* sixty-degree-dense flint prisms and a magnifying power of about 25. The slit was opened until the well defined upper edge of *b* just touched the lower edge of b_2. Then, the spark producing the magnesium spectrum being suppressed, the bright wire of the micrometer was set upon the lower edge of the comet band; finally, the spark being restored, the distance was measured from the edges of b_2. In this way twelve readings were obtained by Mr. McNeill and myself, all giving results ranging between 5160.0 and 5169.5, the mean being 5164.8 ± 0.7. I do not think the possible error can exceed 2 divisions, or three times the probable error. If so, the Comet spectrum cannot possibly be identified with the *second* Carbon spectrum, (the spark spectrum of a Geissler tube containing C O). Since the corresponding band of that spectrum has a wave-length of 5198.4. The wave-length of the band in the flame spectrum is 5165.3—both according to the figures of Dr. Marshall Watts, given in *Nature*, vol. 20, page 28.

As a further test, on July 3d, the suggestion of Dr Watts, made in the paper referred to, was followed out by confronting the comet-spectrum by means of an occulting bar, directly with Geissler tubes containing CO and CO_2, and with the Bunsen-burner flame. The bands on this night were more distinctly defined than on previous occasions, though the nucleus spectrum was less brilliant, and the result of the confrontal was very satisfactory and decisive. The upper and middle bands were found undoubtedly coincident, so far as the power used could show, with the flame spectrum, and *not* with the bands of the tube spectra. In the case of the lower band, the evidence was less conclusive, because the edge was ill defined and faint, making pointing difficult, and because bands of the flame and tube spectra are nearly coincident here. Still, even with this band, the evidence of about half a dozen pointings turned in the same direction.

On the whole, I consider it now absolutely certain, that the comet-spectrum is not the *second* spectrum of Carbon, whether it be the *first* or not. As to this latter point I do not feel quite sure, but the coincidences are certainly very remarkable and close, hough the peculiar appearance of the upper and lower bands when the comet was brightest requires explanation. C. A. YOUNG.

PRINCETON, *July* 4, 1881

PRIMORDIAL COSMIC RINGS.

III.

BY EDGAR L. LARKIN.

The doctrine that a sphere of atoms, abandoned rings, or any other shaped masses can develop into planets is a physical error. It is impossible that the ball revolved. Face the south, hold the plane of the page of "SCIENCE" horizontally, call the paper the centre of the sphere, and it will be seen that to cause rotation, force must be applied, if above the centre, from west to east; below, from east to west; to the right, from below, upward; and to the left, from above, downward. The gas was of excessive tenuity, and external force instead of causing rotary motion would displace the atoms in front of it. The mildest, or most violent force alike, would be unable to cause revolution in a globe of atoms of such extreme mobility. But there was no external force; energy is a property of matter, and the nearest matter was 20 trillions of miles away. If the sphere rotated the motion came from internal causes, none of which could have at that time existed. There were no vortices, currents, tides or whirlwinds in matter of such rarity; no force outside, and none within save the slowest possible radial descent. The sphere was at rest. No point in the experiment of M. Plateau had analogy to the generation of rings on the gaseous globe. He placed a globule of oil in a fluid having like specific gravity, passed a wire through it, and turned it as an axis until the sphere of oil partook of the rotation, flattened and detached a ring. The cosmical mass was of rare gas, and existed in a void, with no external power to turn it. If Plateau had suspended a ball of hydrogen in a vacuum, annihilated the attraction of the earth, and then made it revolve without applying force, the cases would be similar.

Neglecting the laws of Nature we will assume that the primitive sphere was in rotation. Admitting it, a demonstration will be made that if by unknown law it cast off a ring or any other form of mass, said portion could not have been abandoned anywhere in the vicinity of the orbit of Neptune.

First proposition:—If the sphere by rotary motion, or other mode of force cast off its equator, matter which condensing made Neptune, then that planet formed, and now moves on a line that coincided with the *Centre of Gravity* of the discarded mass, no matter what was its shape, size or density.

This statement we deem self-evident, incapable of argument, and an absolute truth.

Second proposition:—If the ring that contained the matter now existing in Neptune, was thrown off the equator of a sphere, a section of the ring perpendicular to its length would be either a circle, or a segment of a circle. That is, the ring would be either cylindrical, or flat inside and curved outside, the curvature being the arc of a great circle, a meridian bisecting the poles of the sphere. We can conceive of no form of mass capable of being detached from the circumference of a sphere. other than cylindric or segmental.

Third proposition:—If the Neptunian ring was not cast off when the mass was a sphere, it was abandoned after the ball had depressed at the poles, and elongated at the equator. And a perpendicular section of such detached protuberance would be some one of the Conic Sections.

Draw a chord of an arc from north to south any distance below the orbit of Neptune, so that it does not de-

scend farther than half way to Uranus, or 500,000,000 miles; then all the matter alluded to in this paper will be above the chord. So long as the mass remains spherical the chord will cut out a segment of a circle. Now let the cosmic sphere receive some unknown impulse that will accelerate its velocity of rotation, and the mass will change to a spheroidal form. The chord of the arc will shorten, matter at the equator will become elevated and sections of the protuberance will change curvature. Make both ends of the chord points of tangency, and produce tangents to the curve to infinite space. Then if rotation accelerates, the curve bounding the ascending equatorial protuberance must continually change form, and the tangents, direction; while sectional curves will pass all varieties of the hyperbola, parabola and ellipse. Thus let the mass become very oblate, pass a cutting plane down to the chord, and the curve cut out will be hyperbolic. Increase rotation; the equator will become higher, the chord shorter and the sections parabolic. Let the velocity be still accelerated, the equatorial matter will be lifted to greater altitudes, the chord will be shorter than ever, and the sections elliptical. To this reasoning the objection may be raised by some that no matter how flattened the mass might become, sections cut to the chord of the arc would in every case be elliptical. We do not insist that they would be hyperbolas or parabolas; but will prove if ellipses, that elliptical segments are more fatal to the theory of ring formation than are segments of any other form of curve. Two factors engaged in the evolution of cosmic rings—gravity and an opposing force generated by rotary motion. We attack the whole Nebular Hypothesis with the fact that if the revolving gaseous mass abandoned matter at present existing in Neptune, the planet is now in the position of the centre of gravity of the detached portion. This being true, Neptune never became a member of the solar family by displacement of its material from the original mass, because no mass could have been cast off whose centre of gravity coincided with the orbit of that world. Let us see if the Neptunian ring was abandoned when the cosmic mass was a sphere. If so, the ring was either cylindrical or a segment of a circle. But the centre of gravity of a section of a cylindric ring is in the centre of the section. Since Neptune now traverses a path once the centre of gravity of the ring, it follows that when detached the spere of gas was larger than a ball bounded by the Neptunian orbit, as there must have been as much matter above the centre of a section of the ring as below. The larger the sphere the slower the rotation, hence it did not rotate as rapidly as it would, had it been equal in size to a globe having the diameter of Neptune's track. But it had to revolve faster to detach a ring because Neptune now moves on an orbit with a velocity of 3.36 miles per second; yet displays no tendency to leave it on a tangent. And greater detaching force would have been required to cause a ring to leave the equator than would now be necessary to throw Neptune off its orbit, because the force had to overcome what little cohesion the dissociated atoms had. The sphere must have been far larger than the path of Neptune, because the ring, being abandoned at the equator, had to be hundreds of millions in thickness to secure gas enough to condense into the planet, and its rate of rotation proportionately less than its present velocity.

It is certain that the ring whence Neptune was formed was *not* cylindrical. The only other possible form of ring is segmental. The distance of centres of gravity of all circular segments from the centre of the circle can be calculated. The problem resolved itself into this:—given the distance of the centre of gravity of the segment of a circle from the centre, to find the dimensions of the segment, and radius of the circle. We know that Neptune is in the position of the centre of gravity of whatever shaped mass was detached. But it lies on the circumference of a circle whose radius is the distance to the sun. Therefore the circle must have been larger than its orbit to be able to afford a segment having sufficient size to have its centre of gravity coincide with the track of Neptune. In all these computations we take the distance of Neptune from the sun to be 2,780,000,000 miles.—Elements of 1850, Newcomb's Astronomy. The ring of whatever shape is supposed to be detached, severed, straightened, and cut into an infinite number of sections perpendicular to its length. In the case in question, sections are segments of a circle, and we are in search of the radius of the circle whence the segment was cut. We have found the length of the radius to be 3,000,000,000 miles, by means of the formula, $G = \frac{C^3}{12A,}$ wherein

G=the distance of the centre of gravity of the segment from the centre of the circle.

C=the chord of the arc, or base of the segment.

A=the area of the segment.

That is—"Divide the cube of the chord of the segment by twelve times the area of the segment; the quotient will be the distance of the centre of gravity required from the centre of the circle."—Vogde's Mensuration p, 237. Making approximation with a circle whose radius was 2,900,000,000 miles, with chords at different distances within the Neptunian orbit, it was found in two trials that a circle of that radius was untenable. Using a circle having a radius of 3,000,000,000 miles, and chord descending 300,000,000 miles, it was soon found that the centre of gravity of that segment was in distance from the centre equal to the distance of Neptune from the sun. But the chord was 2,600,000,000 miles long! Does anybody believe that a break took place along a line of such length, and 300,000,000 miles below the equator of the sphere? Was detachment possible when the sphere rotated slower than the orbital velocity of Neptune now is, yet shows no signs of elevating to a tangent to its path, though moving with unimpeded force? The first world was not abandoned by the cosmical mass when a sphere.

Could it have been formed from the matter contained in the segment of any other curve known to geometers?

To find the centre of gravity of a parabolic area:—"The centre of gravity is on the axis, at a distance from the vertex equal to three-fifths the altitude of the segment." Peck's Calculus p. 175. Then Neptune, as it is the centre of gravity of the parabola must be two-fifths above the base or limiting plane of the curve, We have made calculation of the altitudes of several possible parabolas, by locating the base at different distances between the orbits of Uranus and Neptune. The following table shows the distances of the limiting planes below Neptune, the altitudes of the segments, above the base,—above Neptune,—and also gives the diameter of the mass on the hypothesis, that it could have been so elongated as to make it possible that parabolas could be cut out of the equator by perpendicular planes.

TABLE I. ALTITUDES OF PARABOLAS. DISTANCES IN MILES.

Distances of Base Below Neptune.	Altitudes Above Base.	Altitudes Above Neptune.	Diameters of Mass when so Expanded.
500,000,000	1,250,000,000	750,000,000	7,060,000,000
400,000,000	1,000,000,000	600,000,000	6,760,000,000
300,000,000	750,000,000	450,000,000	6,460,000,000
200,000,000	500,000,000	300,000,000	6,160,000,000
100,000,000	250,000,000	150,000,000	5,860 000,000
50,000,000	125,000,000	75,000,000	5,710,000,000
25,000,000	62,500,000	37,500,000	5,635,000,000

Should these figures be deemed unsatisfactory, because they relate to sections or surfaces, while actually considering a solid ring, a table of *paraboloids* is inserted. The ring was 17,467,000.000 miles long, cut it in an infinite number of parabolic sections; revolve each

about its axis considered motionless, and an infinite number of paraboloids will be generated, all interlacing throughout the length of the ring. The centre of gravity of a paraboloid is two-thirds the distance from the vertex to the limiting plane.

TABLE II. ALTITUDES OF PARABOLOIDS IN MILES.

Distances of Base Below Neptune.	Altitudes Above Base.	Altitudes Above Neptune.	Diameters of Mass when so Elongated.
500,000,000	1,500,000,000	1,000,000,000	7,560,000,000
400,000,000	1,200,000,000	800,000,000	7,160,000,000
300,000,000	900,000,000	600,000,000	6,760,000,000
200,000,000	600,000,000	400,000,000	6,360,000,000
100,000,000	300,000,000	200,000,000	5,900,000,000
50,000,000	150,000,000	100,000,000	5,760,000,000
25,000,000	75,000,000	50,000,000	5,660,000,000

No tables of altitudes of hyperbolas or hyperboloids have been inserted, as the distances of their gravitation centres differ so little from parabolic segments, that it was not thought best to fill up the columns of "SCIENCE" with useless figures. For those who think the ring could not have been left when sections were parabolic or hyperbolic, we give a table of altitudes of ellipsoids, that is when sections cut to the chord as before, were ellipses. "The centre of gravity of a semi-prolate spheroid of revolution is on its axis of revolution and at a distance from the centre equal to three-sixteenths the major axis of the generating ellipse."—Peck's Calculus, p. 175.

Therefore Neptune is 3-16 above the conjugate axis, and 13-16 below the vertex of the ancient semi-ellipsoid, all the worse for the theory of ring detachment. Consider the ring cut by perpendicular planes descending to the chord, into an infinite number of semi-ellipses. The chord becomes the conjugate; revolve each curve about its semi-transverse axis regarded as stationary, then the ring will be made up of an infinite number of semi-prolate spheroids of revolution, each so nearly coincident with the next as to have the surfaces fail to coincide only by infinitesimal space. The table is computed by calling the conjugate diameter, the chord of the arc, and the semi-axis major, the line reaching from its centre up to the equator, Neptune being in the centre of gravity of the solids of revolution.

TABLE III. ALTITUDES OF SEMI-PROLATE SPHEROIDS.

Distances of Conjugate Axes Below Neptune.	Altitudes Above Base.	Elevations Above Neptune.	Diameters of Cosmic Sphere When so Elongated.
500,000,000	2,667,000,000	2,167,000,000	9,894,000,000
400,000,000	2,134,000,000	1,734,000,000	9,028,000,000
300,000,000	1,600,000,000	1,300,000,000	8,160,000,000
200,000,000	1,066,000,000	866,000,000	7,292,000,000
100,000,000	533,000,000	433,000,000	6,426,000,000
50,000,000	266,000,000	216,500,000	5,592,000,000
25,000,000	133,000,000	108,250,000	5,778,000,000

These tables of absurd figures are inserted to show the hypothesis irrational. No such extension of the mass was possible, and no protuberance could have arisen above the equator able to afford perpendicular sections, hyperbolic, parabolic or elliptic. Nor could the chord become the limiting plane of any parabola, hyperbola or conjugate axis of any ellipse. Yet, the tables are logical deductions from the doctrine of ring detachment, for if the mass depressed at the poles, and elongated at the equator, curvature of radial sections must have assumed all varieties of conics. Since the centres of gravity of all these curves, and solids generated by their revolution are known, the figures are correct if the theory is true. It will be shown in a paper on mass, volume and density, that most of these equatorial elevations could not have contained matter enough to form Neptune.

Is it credible that the primeval mass ever detached rings or any other shaped portions? From the altitudes of these conoids it is seen that to cast off the Neptunian material the rupture in every case took place at depths of hundreds of millions of miles, where cohesion was greatest and rotary velocity least! In all these computations the abandoned masses were considered as homogeneous, as difference in density in a gas of such excessive rarity cannot enter as a factor at depths of a few hundred million miles. It may be said that cohesion in a gas so rare, was not a factor. Granted, then *rotation* was not, since a ball of gas of such tenuity as to have no cohesion, could not possibly be set in revolution. The equatorial edge of the mass could not have become angular, for sections cut to the base would be triangles, whose centres of gravity are two-thirds the distance from the angle to the base, and nowhere near where Neptune exists. Neither could sections have been cissoidal, conchoidal, cycloidal or sectoral, nor of any other similar curvature known to geometry. The surface was not irregular; loose masses did not float above the periphery; the matter was all of the same specific gravity, hence *buoyancy* did not obtain on a mass of dissociated atoms. The mass existed in a void, else external matter by friction would have induced currents from east to west. No modes of energy save rotary force, existed to detach a ring, no internal repulsion, as that had vanished in dissociation. The dogma is beset on all sides with difficulties. When the mass was spherical, matter enough to form Neptune was unable to leave the equator; when elongated, segments of enormous depth had to be left by the shrinking mass, to afford material sufficient to condense into the oldest planet; and the break occurred where it was most difficult to be made, and where the power necessary to make it was the least.

Not only the most complex, but the simplest laws of nature dispute the Nebular Hypothesis. Even primary schools have text books wherein laws are laid down that subvert it! Primers of natural philosophy teach that if a revolving sphere diminishes in diameter, its velocity of rotation becomes accelerated, and the same primers teach that if the diameter increases the velocity diminishes. Therefore, if the primeval gaseous sphere ever revolved, said rotation caused the equatorial diameter to increase in length; but as soon as lengthened the velocity of rotation diminished and the mass again became a sphere, the oscillation always remaining within small limits. The diameter of the mass when spherical was 5,560,000,000 miles; can it be believed that rotation so far gained mastery over retardation as to allow the mass to attain diameters ranging between 6,000,000,000 and 7,000,000,000 miles to detach parabolic segments; and between 6,000,000,000 and 9,000,000,000 miles to abandon semi-prolate spheroidal sections to make up a ring? We are unable to conceive that valid argument can be made in favor of the detachment of matter in any form or volume from the mass. This theory, opposed by every known law of nature has actually been entertained by eminent physicists, geometers and astronomers, fully conversant with these same laws that destroy the doctrine; a thing long noted by psychologists, wherein delusions hold sway over fine minds with greater tenacity than ideas known to be true.

SEISMOLOGY IN JAPAN.—The labors of the Seismological Society of Japan have established the fact that there is a chronic center of disturbance within a radius of a few miles from Yokohama. We are glad we do not reside in the said Yokohama; at the same time, we congratulate the society on the success attending its researches.

THE USE OF WATER AS A FUEL.

By Dr. George W. Rachel.

The results of certain experiments, made with what has been called the Holland Hydrogen Locomotive, have lately been published in several city papers. They are not only of the highest practical importance, but of great scientific interest, so that it appears entirely proper to discuss them from that aspect in this journal.

The fuel used is naphtha and water; the manner in which combustion is accomplished by a peculiar unique apparatus may be shortly described thus:

The principal feature of this new invention is an iron retort having two compartments, one for naphtha and the other for water. The two fluids are conducted into the two chambers by induct-pipes at one end of the retort, while at the opposite side there are two escape-pipes, through which the vapors of the two substances escape from their respective chambers, where gasification has taken place. The two gases are being mixed by passing into a common receptacle, the manifold, and from there they are distributed through three main pipes to the 352 burners. Of these 44 are placed directly under the (four) retorts, while the balance is arranged under the boiler.

The astonishing results obtained by this ingenious apparatus have been the subject of many discussions in various scientific and industrial journals on both sides of the Atlantic. The attacks have usually been directed against the possibility of making an advantageous use of the hydrogen for the purpose of combustion. The explanation that in the Holland retort the principal source of the tremendous heat produced, is due to the combustion of hydrogen derived from the dissociation of the water vapor, has been supposed to be met by the following statement:

The dissociation of the steam must consume as much heat, as is afterward developed by the combustion of the hydrogen.

It was contended that the principle of the Holland method was entirely wrong, implying an error against the law of the conservation of energy which is the fundamental law of the Universe, and therefore this whole matter must be a delusion.

This objection, which looks plausible enough can be shown to be erroneous, as it is based on a misconception, or rather a misinterpretation of this great law of Nature. The error consists in the wrong application of the word heat; the sentence containing the objection to be correct, must read thus:

The dissociation of the steam must require as much *energy*, as is afterward developed by the combustion of the hydrogen thus obtained. Now, it is a fact, that the energy developed by the combustion of the hydrogen invariably takes the form of heat, but the principle of the correlation of forces which forms the basis of this very law, teaches us that it must not necessarily do so during the process of dissociation. In order to fully expose the misinterpretation of Nature's fundamental law contained in the objection above quoted, we may be allowed a few words on the subject of dissociation.

Prof. H. St. Clair Deville, who first succeeded in an ingeniously contrived apparatus to dissociate water vapor into its elements, hydrogen and oxygen, estimates the temperature required for the purpose at 6000°C, probably even somewhere near 8000°C. Prof. Schroeder van der Kolk even places it at a still higher figure, viz.: about 10,000°C. But these figures, it must be well understood, refer to the dissociation of water vapor in the absence of any other element. If, on the contrary, the dissociation is induced to take place in the presence of other elements —notably metals—the dissociation temperature is lowered considerably. Thus, for instance, the dissociation is effected in the presence of platinum, at 1700°C; iron filings, 1400°C; silver, 1000°C, instead of 8000°C.

The question is now: How are we account for this? Prof. Deville in a controversy now going on between Prof. Ad. Wurtz and his school, and Berthelot and himself, on this very subject of dissociation, replies to some objections of his adversaries, as follows:*

It is a well-established fact that the dissociation of water-vapor takes place at much lower temperatures in the presence of certain elements. . . . . These and other examples . . . . prove that the development of heat during the formation of a compound body, does not hold any known relation to its dissociation temperature. Evidently the error is very frequently committed in regard to these processes to confound actual and kinetic energy, actual and latent heat."

The relation referred to in this passage, must, however, in the light of the law of "Conservation of Energy" be one of absolute equivalency; the energy expended on one process—dissociation—must under all circumstances be equivalent to the energy developed during the other process—formation of the compound body, *i. e.* in our case water-vapor.

If, therefore, our experiments show that the temperature of dissociation is lowered in the presence of certain elements, we must look for some other form of energy which supplants the amount of heat saved. What force is it that steps in here and plays the role of a dissociating agent in place of the tremendous heat? The answer is obvious, it is *chemical affinity*, for chemical affinity is the only form of energy capable of such intensity of action. Furthermore, chemical affinity is to a certain extent not directly discernible and measurable, as Friedrich Mohr has shown.†

The irrefutable proof for our assertion lies in the fact that there is in the case under consideration, always formed an oxide of the metal employed. We find the molten silver and platinum covered with thin films of their respective oxides while the iron filings show an amount of oxidation which is—as it is in the two former cases—in direct proportion to the quantity of vapor dissociated. The chemical affinity of the glowing and molten metals to the oxygen of the water-vapor being greater than the chemical affinity of hydrogen to oxygen, they appropriate the oxygen of the steam, and, combining with it, form their respective oxides—thus liberating the hydrogen and accomplishing dissociation.

Deville's above-quoted statement, that there is no known relation betweenthe formation—and dissociation-temperatures of compound bodies must be modified, therefore, in the light of the foregoing observations. What he is pleased to call "Kinetic energy" and "latent heat" is actually nothing else than chemical affinity. The position of Wurtz and his followers, by the way, is untenable; they contend that the two temperatures should be equal,(in accordance with the law of the conservation of energy) and meet the often observed fact that these temperatures differ considerably with the assertion that, as theoretically they should not do so, the observations are wrong. Their interpretation of the great principle contains the same error which the objections to the Holland process are suffering from; they insist that the energy which produces dissociation must take the form of heat and heat only, because heat is the only form of energy met with during formation. They forget that such limited application of this great principle is entirely arbitrary and that the only requirement of the law is that of absolute equivalency, while there is no rule as to the kind of energy required.

I have been somewhat elaborate in my remarks on the subject of dissociation, because the conditions under which the dissociation of steam takes place in the Holland process are the exact counterpart to those which have just been dwelled upon. Instead of the metals, the

*Comptes rendus, 1879.

† Science Vol. I, pg. 244.

carbon of the Naptha-gas reduces the dissociation-temperature.

The fact that under certain conditions carbon has a dissociating action on steam, or—as some put it—carbon may be burned up with watery-vapor, has been known for a long time; the presence of free hydrogen in furnace and generator-gases is due to this circumstance. The difference between the dissociating action of carbon on water-vapor as compared with that of the metals above-mentioned is only one of degree. *The temperature at which it takes place is much lower*.

After a prolonged and careful observation of the phenomena connected with the Naphtha and water-process under consideration, the writer was firmly convinced that the carbon in them plays the role of a dissociating agent, and that the temperature at which its dissociating property asserts itself must be a low one, comparatively speaking. For, in this way only was it possible to account for remarkable results of the HOLLAND heating method.

Unhappily, we were not then acquainted with the experiments presently to be discussed, although the fact privately communicated to us that MR. MOSES FARMER, of Hartford, the well-known philosopher, had found experimentally the temperature at which carbon will dissociate water-vapor to be not much above 900° C., seemed to confirm the position taken.

While our proposal to entrust some able chemist with this investigation was under consideration, we became aware of the fact that the desired experiments had already been made in another quarter of the globe more than a year ago. Thus, fortunately, a doubt of their genuineness, which otherwise might perhaps have been entertained by the opponents of the HOLLAND method, is out of the question.

PROFESSOR ACKERMAN, who is superintendent of the chemical labratory at the *Stockholm School of Mines*, requested one of his assistants, MR. C. G. DAHLERUS, to make some experiments with the view of determining the temperature and other circumstances required for the combustion of carbon with watery vapor. The real aim was to explain the occurrence of free hydrogen in both furnace and generator gases; this fact is, as we said before, well-known to mining engineers.

The apparatus used by DAHLERUS consisted of a tube filled with charcoal, which was heated in a combustion furnace, while steam, generated in a separate boiler, was allowed to pass through it. The temperature was determined by trays of steatite containing pieces of Mayerhofer alloys, with various melting points being introduced into the tube. The gases generated were collected, after having passed through a spiral gas tube in order to condense the steam they contained, and were then analyzed. Every experiment lasted at least two hours before a sample of the gas was taken, the pressure of the steam in the induct pipe being kept as uniform as possible.

The results of these experiments have confirmed the correctness of our position, and have shown that dissociation of watery vapor in the presence of carbon takes place at much lower temperatures than has hitherto been admitted.

MR. DAHLERUS, in giving a table of his results, sums up as follows:

"On examining this table it appears that watery vapor is decomposed at a temperature which is indicated by the alloys as from 450° to 500° C.; *but the temperature may, in fact, not have been higher than* 400° *C., because zinc in the interior of the tube was not fused in any of the first five experiments*."

It is evident that in the Naphtha and water process the conditions, under which the dissociating action of carbon on water vapor takes place, are much more favorable to it than those obtained in the apparatus used by DALHERUS for his experiments.

In the first place the action of carbon in the latter gentleman's apparatus could not but be of a very slow nature, the surface only of the glowing charcoal in the tube being enabled to gasify and act chemically on the steam surrounding the pieces of it. In the process under consideration, however, the whole of the carbon of the naphtha is in gaseous condition and by diffusion the vapor is acted upon simultaneously at every point. Furthermore, this very gasification of the carbon requires a definite, not inconsiderable, amount of heat which in DAHLERUS' apparatus has to be supplied by the steam itself, this being the only substance admitted into the presence of the charcoal in the tube. In the new process, on the contrary, this gasification is effected before the carbon-compounds of the naphtha are mingled with the steam and no loss is therefore experienced in in this direction. But, aside from these details—for the combustion-furnace will probably furnish the wanting heat—the highly important fact is established by these experiments that chemical affinity does, in this dissociation process, supplant heat for the greater part. And, considering the great advantages, above detailed, of the HOLLAND process over these experiments, we are justified in assuming the lowest temperature, found sufficient by DAHLERUS in five of his experiments, as entirely sufficient in the HOLLAND process also. *Instead of* 8000° C., *therefore being required for the dissociation of water, it will here take place at* 400° C.

A gain therefore of, say for convenience's sake, nineteen twentieths is effected; for every particle of hydrogen thus dissociated and liberated, at 400° C., will develope its full 8000° C., on combustion with oxygen, *i. e.*, on being burned up by the draft air. And this saving is accomplished by the supplanting of heat with chemical affinity, the latter performing the greatest part of the work of dissociation.

Nor is this all!

It is necessary to state here that DAHLERUS in pursuing his work had in view also, the preparation of *water gas*, which has been introduced into Sweden by PROF. TORREL, who was one of the commissioners from that country to the Centennial Exhibition at Philadelphia. He therefore endeavored to find the most favorable condition for the production of *water gas*, a mixture of hydrogen and carbonic oxide which is known in this country under various names (*e.g.* STRONG, LOWE and others.)

This explains the following sentence in the conclusions he draws from his results:

"Further we see that the greater the excess of watery vapor the richer in carbonic acid are the gases; or, in other words, *that carbonic oxide is very easily burned to carbonic acid by means of watery vapor*, and that the content of carbonic oxide is increased both by a lessened excess of watery vapor and by the raising of the temperature. The best gas is thus obtained by raising the temperature as high as possible and by a moderate supply of steam."

What DAHLERUS refers to as *the best gas* must be understood to be *water gas* in the accepted sense of the word, viz.: A mixture of carbonic oxide with hydrogen. It is for this reason that he advises the use of a limited supply of steam only; for, if there is an unlimited supply of steam, the dissociation of the same continues and the carbon, instead of being confined to its first stage of oxidation (to carbonic oxide), completes this process and is burned up to carbonic acid. Although the result is by these means a gas much richer in hydrogen—in fact twice as rich—this is not what the manufacturer of water-gas wants. He wants a product that may be used for illumination as well as for heating purposes and, therefore, he does not want an almost pure hydrogen-flame—which is non-luminous, as is well known. But with the HOLLAND process this is quite different; here the manufacture of illuminating gas is effected in a separate automatic arrangement which does not concern us here now. In the process under consideration, therefore, the heating quality of the gases is the only consideration. This, the more so, since there is here no separate gener-

ator from which the gases therein manufactured are led away in pipes to the heating-place. The generator, *i. e.* the HOLLAND retort is at the heating-place, in the fire-box of the locomotive, and the full effect of the carbon combustion is therefore obtained in both cases, whether the dissociation of the steam takes place to furnish oxygen for the first stage of this combustion only or whether the dissociation is accomplished so as to burn up the carbon completely with oxygen derived from the dissociated water-vapor. But there is this great difference: If the carbon derives all the oxygen necessary for its complete conversion into carbonic acid from the dissociation of the steam, there will be twice as much hydrogen liberated as against its conversion into carbonic oxide only, as will be seen from the following statement of the two cases by DAHLERUS:

"When watery vapor burns carbon to carbonic oxide, there are formed from two volumes of watery vapor and one volume of carbon two volumes of carbonic oxide and two volumes of hydrogen; further, when carbon is burned by watery vapor to carbonic acid, there are formed from one volume of carbon and four volumes of watery vapor, two volumes of carbonic acid and four volumes of hydrogen. Consequently the volume of hydrogen in the gases is equal to the volume of carbonic oxide and double that of the volume of carbonic acid."

In connection with these important relations I must, in conclusion, refer to the results of numerous experiments, made with the HOLLAND process, which can only be fully and satisfactorily explained in the light of the previous discussion. They are certainly a most remarkable series of experiments, never before equalled or excelled; the results accomplished by the Naphtha and water process have startled all experts and scientists who have witnessed them, while those who have not seen their actual performance reluctantly admit their genuineness. Yet they are absolute facts, and the possibilities which they have in store are greater than anything that has as yet been reported.

In starting the fire under the boiler of this locomotive, it must be stated, there is first lighted a small tank filled with naphtha, which is placed under one of the retorts in the fire-box. As soon as this retort is thereby sufficiently heated to gasify the naphtha, naphtha-gas is burned under all the retorts, and water admitted into them to be converted into steam. When both naphtha and water are thus gasified, their gases are jointly admitted to all the burners under the whole length of the boiler, and the generation of steam now begins in earnest. As soon as feasible, steam from the boiler is introduced into the retorts instead of water, so that after this period the naphtha only has to be gasified in the retorts.

I now give one of Mr. CONANT'S tables in full, containing the results of an experiment he witnessed on April 29th:

LIGHTED AT 10:05 A. M. GAS STARTED AT 10:35.

Steam, Pounds.	Time, M.	Naptha, Gall.	Naptha, Per Lb. Gall.	Naptha, Per Min Gall.	TOTALS. Gall.	TOTALS. H. M.
10	69½	5.62	.56	.08	5.62	1 09½
20	15	3.83	.40	.27	9.45	1 24½
30	18½	2.7	.29	.15	12.35	1 43
40	9	2.41	.24	.27	14.76	1 52
50	8	2.14	.21	.27	16.9	2 00
60	7	2.14	.21	.30	19.04	2 07
70	5	1.61	.16	.32	20.65	2 12
80	3½	1.07	.10	.30	21.72	2 15½
90	4½	1.07	.10	.24	22.79	2 20
100	4	1.07	.10	.27	23.86	2 24
110	4	1.07	.10	.27	24.93	2 28
120	4	1.07	.10	.27	26.00	2 32
Engine started out—safety valve blowing—oil disturbed and no record.						
133	5	----	----	----	----	----

Pop valve blowing av. 33 sec., with 32 sec. intervals. No right of way and no run.

The puzzling fact that the higher the temperature and the steam-pressure rise, the less naphtha is burned, would be absolutely inexplicable if it was not for the relations alluded to in the foregoing observations. Up to 60 or 70 pounds of steam-pressure in the boiler the consumption of naphtha averages 2.14 gals. for every ten pounds of pressure added, while above these figures, it averages only 1.07 gals.—just one-half of the former quantity—for every additional 10 pounds. We know what that means. It means that there is an evident supplanting of the naphtha by some other much more powerful heating agent; the naphtha in this process unmistakably plays a subordinate role, as far as the heating is concerned. We know its task. It dissociates the water and thereby liberates its hydrogen; it is the latter that furnishes the bulk of the caloric energy developed. During the earlier stages, when the steam-pressure is yet comparatively low, the quantity of steam introduced into the retorts is limited and the carbon therefore is burned up to carbonic oxide only by dissociated oxygen; as soon, however, as the steam-pressure rises above a certain point the quantity of steam introduced is very soon sufficient to furnish all the oxygen necessary for the complete combustion of the carbon of the naphtha to carbonic acid. Thus, we are enabled by a correct interpretation of Nature's laws to explain fully and satisfactorily the paradoxical fact that the greater the heat, the less the consumption of oil. We know that instead of two volumes of hydrogen in the first, we must have four in the second case.

There is one other point which I may probably feel called upon to treat of, viz.: the utter invisibility of this tremendous fire. For the present the above will suffice.

DR. GÜNTHERS ICHTHYOLOGY.*

Less than a century ago the last edition of the Systema Naturæ of Linnæus, published in 1766, was taken as the basis and text of essentially a new compilation by Johann Friedrich Gmelin, and among the species admitted by Linnæus were intercalated those subsequently added by others to the system. There were very many duplications arising from the imperfect acquaintance of the compiler with his subject, but nevertheless, all told, only 826 species of fishes were named. There are now known, in round numbers, nearly ten thousand species. In the interval between the compilations of Gmelin and the present were published works of a like nature, by Walbaum, Lacépède, Bloch, Schneider, and Shaw. These were all finished before 1804, and were all of very little value. For considerably more than half a century no other descriptive general enumeration of fishes was completed. Meanwhile, from 1828 to 1849, Cuvier and Valenciennes gave to Ichthyology 22 volumes of a work designed to be a general natural history of fishes, but this was never finished. At last, in 1859, was commenced and in 1870 brought to an end, a work purporting to enumerate all the species of fishes known to the dates of publication, by Dr. Albert Günther, under the auspices of the British Museum. For this contribution the scientific world was laid under great obligations to the author as well as publisher. It was a compilation requiring considerable skill and acquaintance with the literature, and the work may be said to have been moderately well performed. Its author followed the outlines of classification proposed many years before by the illustrious Johannes Müller. On the whole this was the best course, perhaps, to be taken at the time. In 1861, however, he gave a systematic re-arrangement of the Acanthopterygian families, which was above all characterized by an excessive valuation placed on very trivial charac-

* An introduction to the study of fishes. By Albert C. L. G. Günther. Edinburgh: Adam and Charles Black. 1880.

ters, and was, in some respects, a step backwards, although of not very much moment.

Another and most radical modification—the next stage—may be fitly noticed in the author's own words. [1.] "The discovery (in the year 1871) of a living representative of a genus hitherto believed to be long extinct, *Ceratodus*, threw a new light on the affinities of fishes. [2.] The author who had the good fortune of examining this fish, was enabled to show that, on the one hand, [3] it was a form most closely allied to *Lepidosiren;* on the other, that it could not be separated from the Ganoid fishes, and therefore that also [4] *Lepidosiren* was a Ganoid: a relation pointed out already by Huxley in a previous paper on 'Devonian Fishes,' [5] This discovery led to further considerations of the relative characters of Müller's sub-classes, and to the system which followed in the present work" (pp. 25-26). In regard to this claim there are several noteworthy and characteristic features.

(1) In 1870, in Dr. Günther's Cat. Fishes Brit. Mus., vol. 8, p. 323, it is expressly admitted that "after [the 'sheet' descriptive of *Protopterus* and *Lepidosiren*] had passed through the press, Mr. Krefft informed me of the most interesting discovery that a living representative of *Ceratodus* had been found in Queensland. Nothing of this genus was hitherto known beyond teeth, as those described and figured by Agassiz in Poiss Foss. iii, p. 129, pls. 18-20." (2) Dr. Günther knew nothing whatever of *Ceratodus* till he received a communication respecting it from Mr. Krefft. (3) As indicated by Dr. Günther himself (Trans. Royal Soc., v. 161, for 1871), Mr. Krefft, in even the title of his paper, published April 28, 1870, and before Dr. Günther's "reply had time to reach Mr. Krefft," recognized the affinity of the genus to *Lepidosiren*. (4) As early as 1860, Gill (as Brandt, Peters, Lütken and others subsequently recognized) showed that "*Lepidosiren* was a Ganoid," and that *Polypterus* was a type intermediate between the ordinary Ganoids and the Dipnoi. (5) Consequently the only novelty in Dr. Günther's work was "the system which is followed in the present volume," which has been pronounced by an eminently competent judge to be "a triumph of systematic *gaucherie*." Whatever is true in the statements examined had been appreciated before Dr. Günther labored and only what is untrue to nature and to science was original with him. The co-ordination of the facts enumerated was the necessary logical result of the successive steps.

But what is "the system which is followed in the present work?" Only the salient features may be noticed, and these will sufficiently appear from the enumeration of the sub-ordinal, ordinal and super-ordinal groups. These are:

I. SUB-CLASS—PALÆICHTHYES.
- I. Order—Chondropterygii.
  - I. Sub-order—Plagiostomata [Sharks and Rays].
  - II. Sub-order—Holocephala [Chimæroids].
- II. Order—Ganoidei.
  - I. Sub-order—Placodermi [Extinct].
  - II. " —Acanthodini [Extinct].
  - III. " —Dipnoi.
  - IV. " —Chondrostei.
  - V. " —Polypteroidei.
  - VI. " —Pycnodontoidei [Extinct].
  - VII. " —Lepidosteoidei.
  - VIII. " —Amioidei.

II. SUB-CLASS—TELEOSTEI.
- I. Order—Acanthopterygii.
- II. " —Acanthopterygii Pharyngognathi.
- III. " —Anacanthini.
- IV. " —Physostomi.
- V. " —Lophobranchii.
- VI. " —Plectognathi.

III. Sub-Class—Cyclostomata.

IV. Sub-Class—Leptocardii.

To those familiar with the facts and details of the anatomy of fishes and the inferior vertebrates, this enumeration will be its own best commentary. Suffice it for the present at least to affirm that it involves more contradictions and inconsistencies than have been manifested in any recent taxonomical exposition of any class of animals emanating from a respectable source.

Almost equally in disaccord with the cultivators of the other branches of Vertebrate Zoology is Dr. Günther in his treatment of GENERA.

The extreme of differentiation is practiced by ornithologists, (provided the differences are obvious and external), and a course is pursued in mammalogy which has received the sanction of the greatest number of students of that class, during at least the last quarter of a century. American ichthyologists have endeavored to comply with the principles on which genera in the latter class have been recognized as much as the differences of facts will permit, and although, of course, there are many disagreements as to detail, there is an essential congruity between them. The principles, if any, applied by Dr. Günther are undiscernable from his work. His methods indeed, seem to have varied with the whim of the moment and to have been modified for each case: the results then happening appear for him to have crystallized and not to have been subject to review or further consideration afterwards. Strange contrasts constantly occur in the extension or limitation of the groups. In the genus *Tetrodon*, for example, is discoverable a very considerable range of variation, not only in external features but still more markedly in the details of structure, and especially in the bones of the head. So great are these that there are three well defined major groups and a number of minor ones entitled to generic distinction, but, nevertheless, our author has refused to admit more than one "genus" for all the representatives of the type, whereas, in the related group of Diodontines, he has recognized a number of genera upon characters of very much less moment, such as the development of the spines, nostrils, &c. Under the genus *Gasterosteus* are confounded all the representatives of the family of Gasterosteids, and yet upon differences of the same kind as those which distinguish, for example, the "*Gasterosteus spinachia*" from the other species of *Gasterosteus*, are elsewhere constituted distinct *families*.

These examples might be extended indefinitely. Heterogeneous combinations of forms on one hand chance in strange contrast with isolated generic types on the other.

Comprehensiveness of genera *per se* is not a great evil, provided there is consistency in the treatment of the subject, and that all share as nearly alike as the nature of the case allows. It is to the assignment of inordinate value to a few superficial characters, and the subordination, to the manifestation of such, of other characters whose coincidence demonstrates them to be of greater importance, that we object. It is true that the acceptance of such comprehensive groups isolates in a measure the class in which they are recognized from others and tends to constantly mislead the inquirer who would compare the constituents of the several classes, *e. g.*, as to their geographical or geological relations. Even this, however, is of minor importance. It is the utter disregard of the gradations of structural differences exhibited by Dr. Günther in his constitution of genera that detracts so much from the value of his work. To enter into detail would necessitate space equal to the portion considered, and some instances must suffice.

Serranus (p. 381) is distinguished among its allies by the "small scales," presence of "very distinct canines in both jaws," and the absence of serratures from the lower margin of the preoperculum. Under the genus thus defined, there are not only species which disagree with the principal characters, but the *typical Serrani (S. cabrilla, S. scriba*, etc.) are more nearly related to the species of *Centropristis* than to the rest of their associates. A

natural arrangement—*i.e.* one based on their anatomical details—would require, first, the fusion of the Güntherian genera *Centropristis, Anthias, Callanthias, Serranus, Anyperodon, Prionodes, Plectropoma,* and *Trachypoma*; then the wide removal of certain forms, and finally the disintegration of the conglomeration on an entirely different basis from that accepted by Günther.

The instances wherein genera are referred to families with the diagnoses of which they diametrically disagree are numerous. Leaving out of consideration cases of conflict of genera or species with the characters assigned as *ordinal* to the including group (e. g., *Pogonias, Sciæna, Gerres*) the following are examples:

The genus *Dactyloscopus* is referred to the family Blenniidæ, in which the spinous portion of the dorsal fin is said to be "as much developed as the soft, or more." *Dactyloscopus* has in the most evident manner, notwithstanding the erroneous definition of Günther (Catalogue of the Fishes in the British Museum, Vol. III., p. 279), only the first ten to twelve dorsal rays spinous, all the others being articulated. In fact, *Dactyloscopus* has nothing whatever to do with the Blenniidæ, but is very closely related to *Leptoscopus*, and belongs unquestionably to the same group; in other words to an entirely different division of fishes in the Güntherian system. (See Trachinidæ p. 462.)

The genus *Zoarces*, (p. 497) also referred to the family of Blenniidæ, still more disagrees with the true representatives of that family in the structure of the dorsal fin and, as he himself admits, has "no other fin spines" than a few near the caudal; it shows, in fact, an organization similar to that manifested in the family Lycodidæ of Günther, (p. 537) placed by him in a different *order* of fishes—the Anacanthini.

Siphonognathus is a remarkable genus referred to the family of Labridæ. This family is defined as having, in addition to other characters, "the soft anal similar to the soft dorsal, ventral fins thoracic, with one spine and five soft rays," and "branchiostegals five or six." Nothing whatever is said respecting the anal, ventrals, or branchiostegals of *Siphonognathus* and as the necessary data are thus entirely suppressed, it would naturally be assumed that the genus would have the characters attributed to the family. In fact, however, *Siphonognathus* has not the "soft anal similar to the soft dorsal," there are *no* ventral fins, and there are only *four* branchiostegal rays. It will be thus apparent that it would be impossible to identify this fish from Dr. Günther's Introductiou, unless it were assumed that great blunders had been made. This is indeed the case, but it is not safe to assume that the author is an habitual blunderer, and to proceed on that basis, even in the case of Dr. Günther. We are somewhat prepared, however, for the idiosyncrasy exhibited by Dr. Günther, when he compares the relationship of *Siphonognathus* to *Odax* as being similar to that of *Babirussa* to *Sus* (see Catalogue of Fishes in B. M., v. 4, p. 243). Any one who can really entertain such views, and consider the differences between the mammalian genera to be of the same kind or degree as those between the fish genera is unfit to institute comparisons.

Numerous genera are adapted, which, although they may be good, consistency would require Dr. Günther to merge with others. Thus we have *Ptyonotus* (which he has unnecessarily substituted for *Triglopsis* of Girard) retained for a form in the family of Cottidæ (p. 480); this is, however, far more closely related to the "*Cottus quadricornis*" of Günther than are any of his other species of that heterogeneous group. *Pammelas* is still retained as the name of a distinct genus which is allied to *Trachynotus*, although it had been named before Dr. Günther applied his, and its affinities have been well-known for many years to be with *Centrolophus*: it is indeed to a species of that genus (the *C ovalis*), that the *P. perciformis* is most closely related, and yet in spite of the concurrent testimony of previous ichthyologists we find it injected, in the "Introduction to the Study of Fishes," into a family remote from that to which *Centrolophus* has been referred. As examples of other forms unnaturally separated we may instance (1) *Chætopterus* (p. 390) and *Aprion* (p. 397); (2) *Grystes* (p. 392) and *Huro* (p. 393), and (3) *Auliscops* and *Aulorhynchus* (p. 508). The last type, it may be remarked, is more nearly related to the so-called *Gasterosteus spinachia* than to the Fistulariidæ and should be either referred to the same family or differentiated as a distinct one.

Changes of the names of established genera on trivial pretexts are also indulged in. The name of *Triglopsis* was abandoned for *Ptyonotus* because there was a *Triglops* previously established. Although they are unquestionably much alike, they are sufficiently different, and Steindachner has even lately named a genus *Atherinops*, knowing well that *Atherinopsis* had already been proposed for another genus of the same family. *Dactylopus* is discarded for *Vulsus* because, forsooth, the term DACTYLOPODA had previously been applied by Meyer to a group (not genus) of extinct reptiles. And yet our author himself retains both *Chondrosteus* and Chondrostei etc., without the slightest demur. *Xiphasia* is rejected with an exclamation mark (!) and the yet more objectionable name *Xiphogadus* proposed because the author was dissatisfied with the name,and—we strongly suspect—still more with the namer (Swainson). Why expect any better reason?

The idea is conveyed in the work—and that it has been extensively claimed elsewise by our author is no secret—that all the established genera are admitted in this volume. Without counting the scores of genera that Dr. Günther refuses to recognize, but which every one applying the canons observed by mammalogists and ornithologists would adopt, there are many which even that author could scarcely neglect unless through ignorance. Among those omitted, and which are especially interesting, on account of representing previously unknown types of high value (families or sub-families), or because they throw light on the relations of families in which they belong are: *Elassoma, Xenichthys, Hoplopagrus, Gnathanacanthus, Nematistius, Grammicolepis, Bathymaster, Cottunculus, Oxylebius, Anoplopoma, Dactylagnus, Myxodagnus, Anarrhichthys, Plagiotremus, Chænopsis, Nematocentris* and *Protistius*. If he had really known *Hoplopagrus* (referred to incidentally on page 279, but not otherwise noticed), he, *perhaps*, would not have so far separated his "*Percidæ*" (pp. 375–379) and "*Sparidæ*" (405-410), as he has done: if he had known *Cottunculus* he would, *perhaps*, have recognized the affinity of *Psychrolutes* to the *Cottidæ*, and not isolated it as the type of a remote family—at least no scientific ichthyologist would have failed to so profit by the knowledge. The work of Bleeker, Steindachner, Klunzinger, Lütken, Vaillant, Sauvage, Giglioli and Collett in Europe, and that of all American ichthyologists has, however, been almost of nought so far as Dr. Günther is concerned. It need be only remarked, in connection with the latter, that of the numerous genera of Etheostomine fishes only *Pileoma* (*Percina*) and *Boleosoma* (p. 379) are recognized. The reason therefore is no secret—they are too small, and as they have not been able to grow larger, they do not deserve to be considered. The interesting relations, physiological and morphological, that they present are not sufficient to outweigh this cogent objection. Among American fishes there is no group that has been so much written about and that is better known than the genus *Micropterus*, but notwithstanding Dr. Günther has not yet learned that he has distributed its well defined representatives under three genera, nor that *Huro* was based on a mistake and is not a valid genus, nor that there are two, and only two, well-determined species, and those two can not be generically distinguished. When it is further remarked that only three genera are recognized

for the Centrarchines and Lepomines, and that these are diagnosed by the least important and most fallacious characters, and that thereby the species are thrown into almost inexplicable confusion, some idea may be formed of the unreliability of the work.

The general anatomical portion of the work is, on the whole, really a tolerably good *résumé* of facts respecting the structure and organization of fishes, for the author has wisely followed Gegenbaur, Huxley and Parker without sufficient deviation to fall into much error. One great objection to it, however, is the undue prominence given to the peculiarities of the teleostean types and the exhibition of them in such a manner as to prevent the reader's conception of the range of variation in the forms treated of, and especially as to the taxonomic value of such variations. In this connection too, we may notice the reproduction of some rather strange views. Thus, it is said that "the numbers of the dorsal and anal rays give good specific, generic, or even family characters," except when greatly increased, while "the taxinomic [taxonomic] value of this character becomes uncertain. The numbers of the pectoral and caudal rays are rarely of any account" (p. 44). The last remark embodies a striking illustration of the length to which Dr. Günther's neglect carries him in contempt of the facts. Far from the number of the completely developed caudal rays being of no account, there are rarely deviations in the number in related forms, and when such prevail they generally accompany other decided modifications of structure and are available for major diagnostic purposes, as Bleeker has observed. Again, it is claimed of the pectoral limb that the structure of that of *Ceratodus* "*evidently*" represents one of its first and lowest conditions" (p. 74). So far is this from being "evident" that it is difficult to understand how any one familiar with the stucture and development of the limb in the Selachians and related types, and conversant with the logic of science could entertain for one moment such an opinion and, on the contrary, not look upon the Ceratodontoid limb as an extreme deviation from the primitive type. But the very climax of absurdity and unscientific comparison is exemplified in the case of *Ceratodus* by the homologisation of the basal segment of the axis of the pectoral *fin* (not that which supports it) with the basal cartilage of the Sturgeon, and which itself is the source of several other errors (pp. 74, 76). A comparison of the pectoral limbs of *Ceratodus* and *Polypterus* would be sufficient to prevent any scientific naturalist from making such a blunder. We need not dwell further on such defects but in connection with the systematic portion, we cannot omit to notice that Dr. Günther recognizes that in the Chondropterygians there are no bones representing the membrane bones of the skull of the Ganoid and higher fishes; that at the most there are simply "rudimentary maxillary elements" (p. 69); that the scapular arch "is formed by a single coracoid cartilage" (p. 69); that "the same type of branchial organs [as in the Cyclostomes] persists in *Chondropterygians*, which possess five, rarely six or seven, flattened pouches with transversely plaited walls," each pouch opening "outwards, and by an aperture into the pharynx, without intervening ducts" (p. 137); and that an "air bladder is absent but occurs in all Ganoids," etc. (p. 141), and that the generative organs are very peculiar (p. 166). Yet in spite of all these differences, in face of the recognized similarity between the teleosteoid Ganoids (*Amia*, &c.) and certain Physostomes, and in ignorance of the evanescence of the characters designed to differentiate the Teleosts, he adheres to the combination of the Ganoids with the Chondropterygians in one sub-class—the Palæichthyes. It is indeed a "singular concurrence" of characters (p. 312)—but not of important ones—that is employed to segregate this group, for not one is common to all the members included in it, and at the same time exclusive of other types. A knowledge of the anatomical labors of recent biologists would have instructed him as to this fact. The "Sub-class Palæichthyes" is indeed, as has been said by a recent well qualified judge, "a triumph of systematic *gaucherie*." The group in fact is the outcome of a confusion of ideas respecting generalized characters and extravagant valuation of certain facts entitled to consideration but by no means to anything like the extent admitted.

Quite as inscrutable as his Morphology is Dr. Günther's Physiology. As we turn the pages of the Introduction we come across strange assertions respecting the functions connected with structural peculiarities. Several of these may be taken as examples.

The power of ejecting from the mouth drops of water to some distance, and with such force as to dislodge insects and precipitate them into the water, has been attributed to more than one Javanese fish, but whether the real shooter was a *Chelmo*, a *Toxotes*, or an *Epibulus*, or each one, (or even whether any actually had such power), seems to have become doubtful. Skepticism as to any case might have been legitimate, but Dr. Günther unqualifiedly asserts that as to *Chelmo* "*this statement is erroneous*," and that the feat "*is practised* by another fish of this family (*Toxotes*). The long slender bill of *Chelmo* (which is a true salt-water fish) rather enables it to draw from holes and crevices animals which otherwise could not be reached by it" (p. 399). *Toxotes* has an unusually deeply cleft mouth, and one less fitted to perform such a feat as that in question could scarcely be found. The inaptness of the structure to the alleged function might well evoke skepticism in anyone, and this being once excited, the literature respecting the several fishes which have been named ejaculators will demonstrate that (1) there is no *observational* basis for the attribution of blowing drops of water to the *Toxotes*, and (2) there have been observations (by Hommel, Reinwardt and Mitchell), of a certain kind, of ejaculatory feats by *Chelmo*. In fact, if it is conceded that the feat is performed by a fish, in the sentences repeated from Dr. Günther, there are concentrated seven distinct errors: (1) denial in spite of evidence, (2) affirmation without sufficient basis, (3) denial in face of (comparative) adaptation of structure to function, (4) credulity in spite of inaptness of structure to function, (5) gratuitous assumption of a function—"to draw from holes and crevices animals which could not otherwise be reached by it," (6) the assumption, by implication, that the Archer was not a salt-water type, although the first observer (Hommel) especially stated that it was a sea-fish, and (7) erroneous taxonomy in the association of *Toxotes* in the same family with *Chelmo*. Almost all possible kinds of errors have thus culminated in this single case.

An instance of another gratuitous assumption respecting a function, refers to a Sciænoid fish.

The genus "*Collichthys* Günther" (previously named *Sciænoides* by Blyth) is distinguished by a "great development of the muciferous system on the head and the small eye," and this characteristic "leads one [and but one—Dr. Günther alone] to suppose that these fishes live in muddy water near the mouths of large rivers" (p. 430). What teleological relation there is between muciferous channels and small eyes and the muddy water of large (or any kind of) rivers, Dr. Günther has not vouchsafed to inform us. That such characteristics do not usually indicate the conditions suggested, is admitted by Dr. Günther himself, for he has recognized that "the muciferous system of many deep-sea fishes is developed in an extraordinary degree" (p. 300), and that a large portion of the deep-sea forms are characterized by small eyes (pp. 300-301). The fact is that instead of the inference in question being the outcome of a consideration of the structure indicated, it is the result of data concerning the habitat of one species of the genus and the desire to connect the structure with some function, however irrelevant. It is recorded in the "Catalogue of the Acanthop-

terygian Fishes in the British Museum" (v. 2, p. 316)—a work which has served as the basis of the "Introduction to the Study of Fishes"—that the "*Collichthys pama*" inhabits the "Bay of Bengal, entering rivers." The statement given as a deduction is therefore really a co-ordination—and an entirely sophistical one—of the ascertained structural peculiarity and the habitat of that species.

One other characteristic deduction, also relating to a Sciænoid type, may be noticed because of its interest to American students.

The "Drum" of the Atlantic (*Pogonias chromis*) is especially mentioned in connection with "the extraordinary sounds which are produced by it and other allied Sciænoids." "It is [says Dr. Günther] still a matter of uncertainty by what means the "Drum" produces the sounds. Some naturalists believe that it is caused by the clapping together of the pharyngeal teeth, which are very large molar teeth. However, if it be true [sage proviso!] that the sounds are accompanied by a tremulous motion of the vessel, it seems more probable that they are produced by the fishes beating their tails against the bottom of the vessel in order to get rid of the parasites with which that part of their body is infested." In this paragraph are several illegitimate assumptions and inferences which a slight knowledge of the literature respecting the subject would have prevented. (1) The sounds are entirely independent of "vessels." (2) There was no reason to suppose that the fish in question was more infested with parasites in the tail than any other. (3) The statement that "allied Sciænoids" (and this is especially true of the closely related fresh-water sheepshead, or *Haploidonotus*, referred by Günther to a genus with which it has not the slightest affinity!) produce similar sounds was for the moment forgotten. (4) The co-ordination of facts and phenomena rendered it unnecessary to look to such source for solution. (5) The source indicated was one of the most improbable that could be conceived. There is, indeed, ample cause for surprise that any educated ichthyologist could suppose that a fish would agitate its tail in the manner suggested to relieve a spasmodic pain, such as is postulated by the explanation given. Our author's credence in the allegation that the sounds produced are "accompanied by a tremulous motion of the vessel," was, as we have seen, sufficient to impel him to substitute a most improbable for at least a probable hypothesis.

A mistake of another kind is made respecting the Rays. It is said that "the majority are *oviparous*" (p. 336). As was long ago recognized by Müller and Henle, the Raiidæ are the only oviparous rays; Gunther includes them all in one family and four genera, and admits about 35 species. All the others recorded by him, so far as known, are viviparous; they number, in his opinion, five families, twenty genera and more than 100 species, consequently the majority are *viviparous!*

Whether a work so abounding in errors that we are only able to specify a few as examples and hint at some kinds of others is worth acquiring must be left to the reader to judge. As a curiosity in taxonomical literature it certainly *is*, but for such purposes as are most desirable—correct information and identification of genera—it as certainly *is not.*

THEO. GILL.

DESCARTES AND THE BAROMETRIC THEORY.—At one of the late sittings of the Academy of Moral and Political Science, M. Nourisson made an extremely interesting communication relative to a letter of Descartes, in which the great philosopher clearly indicates the principal of atmospheric pressure, twelve years before Toricelli's experiments on the barometer. Toricelli constructed the fast barometric tube in 1643; in 1647 Pascal accomplishes his celebrated experiments of Puy-de-Dôme and of the "Tour Saint Jaques." It would appear that Descartes had suggested to the author of *Pensées* the idea of this mode of experiment.

CORRESPONDENCE.

To the Editor of SCIENCE:

ON ETHER.

There are two theories in regard to ether, one of which assumes that it is a discontinuous medium, that is, a medium composed of particles at enormous distances apart, as compared with their diameters.

In this theory ether is spoken of or defined as an "imponderable elastic medium." If we examine the above definition we find several inconsistencies. To begin with, an imponderable body is a body without weight. Now the weight of a body, is the result of the mutual attraction, exerted between it and some other body; in other words, weight is the effect of gravitation. Now as every particle attracts every other particle with a force, that is directly as the mass, and inversely as the square of the distance between them, an imponderable body must be one in which the mass is zero, or that is at such a distance from every other body that the reciprocal of the square of this distance is zero. The last supposition is of course absurd.

Now the mass of a body is equal to the product of its volume and density, or $M=d\ V$ and if M is equal to zero, either d or V must be zero and as it would be impossible to conceive of a body that occupies no space, we must think of d as equal to zero, or in other words an imponderable body is simply a portion of space. This same theory assumes that radiant energy is transmitted by means of the moving particles of ether, *i. e.*, one particle moving with a certain velocity, strikes another and imparts some of its energy to it and this flying off strikes another and so on. But the momentum of a body is expressed by $M\ V$ and its energy by $\frac{M V^2}{2}$ (V=Velocity), making M equal to zero, as we must if the particles are imponderable, we have $O\ V=M_0=o$ and $\frac{O\ V^2}{2}=E=o$, hence the transmission of radiant energy by an imponderable substance, composed of particles is an impossibility. If we assume that the particles are effected by gravitation, then at once it is evident that the ether could not be of equal density throughout the universe, for around each celestial body there would be an atmosphere of ether which would gradually decrease in density from the surface of the body outwards.

By elasticity in the above definition, is meant that property of matter, possessed by gases in the highest degree, of having its volume or density changed by some force and regaining its former state when the original condition are again imposed. When a gas is compressed, the mean free path of the molecules is shortened and the compressibility is dependent upon the length of the mean free path. When the pressure is removed, the gas expands, the expansion being due to a conversion of the energy of vibratory motion of the molecules or heat into energy of translation. If the ether is elastic, then of course with a change from less to greater density the particles must be moved nearer together, and the compressibility will be dependent upon the average distance between the particles. When a change from greater to less density takes place, the particles must be moved farther apart and the explanatory reason given for this expansion is that the energy of the moving particles causes the expansion.

From what has been said in regard to imponderability, it is evident that a discontinuous imponderable elastic substance is an impossibility according to the present ideas of dynamics. The transmission of radiant energy by a discontinuous ether, if the particles are ponderable, is possible in two ways, 1st, By an alternate rarefaction, and condensation of the ether, similar to the manner in which sound is transmitted through air. 2d, By the

movements of individual particles. If the first is true, the same conditions must apply to the transmission of radiant energy, as to the propagation of sound. Sound travels through air with a velocity of 330 miles per second, at 0° cent.

Taking for an example a sound produced by a body vibrating 20,000 times per second and dividing the velocity of sound by this number, we have as the wave length 16.5 mm. Clausius has shown that the mean free path of an oxygen molecule is 5000 times its diameter. Taking $\frac{1}{5 \times 10^7}$ mm. as the diameter, we have 10^4 mm. as the mean free path of an oxygen molecule, and dividing the length of the sound wave calculated above, by this number we have 1665 x 10^4, or the length of the wave of this extremely high note, is 1665 x 10^4 times longer than the mean free path of an oxygen molecule, hence it is evident that the propagation of sound is dependent primarily upon the movement of aggregates of molecules.

The elasticity of the ether is assured to be many times greater than that of the most perfect gas. Assuming that it is 1000 times more elastic than oxygen gas, the average distance between the surfaces of the particles must be 1000 times greater than the average distance between the surfaces of the oxygen molecules.

Taking as the mean free path of an oxygen molecule, 10^4 mm., the distance between the particles of ether would be .1 mm. Now the wave length of a certain ray of red light is .000,609 mm., hence the average distance between the particles is 164 times as great as this particular wave length. It follows from this, that the transmission of radiant energy, through such an elastic medium as the ether, cannot be in any way comparable with the propagation of sound through air. If the energy is not transmitted in this manner, then it must be transmitted in the second way, *i. e.*, by the movements of individual particles. But with an ether as elastic as generally assumed, this is impossible, since the average distance between the particles is 164 times as great as the length of a comparatively long undulation, and it would be absurd to say that a vibrating molecule could, by impact with a particle of ether, send the particle 5000 times the diameter of the molecule, and further, that the particle would return from this long journey in time for the next vibration. Even assuming that the particles of ether could move fast enough to accomplish this movement in each vibration, then if the molecules are circular, the particle would have to return in a line that was normal to the surface of the molecule at the point of contact, or it would fly off in another direction after impact with the molecule, and as the particles are so far apart, as compared with the diameters of the molecules, if one particle was driven off there would be no other to take its place. There would also be required a series of particles in a straight line between the body receiving and the body radiating energy.

But it is needless to enlarge upon this method of transmitting radiant energy, for the constant length of undulation and undulatory motion itself, would be impossible in a medium, in which the average distance between the particles was 164 times as long as an undulation.

The only discontinuous medium through which radiant energy *could* be transmitted, would be one in which the average distance between the particles was a small fraction of an undulation. But in a medium of this sort there would be hardly any chance for compression, much less than in oxygen gas, and to assume that ether is less elastic than a gas, is contrary to the theory of discontinuous ether. As a discontinuous ether will not answer the requirements, we must, if we assume *any* ether, assume a continuous one. By means of a continuous ether all the phenomena of light can be explained. One is inclined, however, to apply to a continuous ether the same reasoning as is applied to matter. But as ether is not matter, we cannot with justice attribute to it any of the properties of matter except extension and elasticity, and till we are much farther advanced in our knowledge of the universe, it will be impossible to say anything about ether, except to assume its existence and its continuity. B.

THE COMET.

The comet was seen from this Observatory at 14h. 30m., June 22, 1881. The latitude of the place is 41° 13′; longitude from Washington in time, 53m. 48s. This longitude is approximate, as we have no transit, and being without a correct astronomical clock, are continually annoyed for want of true time. The latitude is somewhat indeterminate, as the declination circle has no Vernier, reading seconds. The telescope (a fine 6 inch Alvan Clark & Sons), is not precisely in the meridian, and we are unable to place it there with accuracy, having no micrometer. With all these hindrances, the adjustment is such that catalogued stars are always in field with power of 60, but in many cases fail to come to the centre or line of collimation. When observation was first made the declination was 43° 10′, then make δ=the declination = 43° 10′, and λ = the latitude of New Windsor = 41° 13′, and take:

log. tan. δ = - - - -	9.972,188
log. tan. λ = - - - -	9.942,478
log. sin. 55° 15′= - - -	9.914,666

which being converted into time = 3h. 41m.; and 6h. — 3h. 41m. = 2h. 19m., A. M., June 23, mean local time in New Windsor, or time of comet's rising, that is, of the nucleus. The tail being several degrees long and direected towards Polaris, was above the horizon some time before.

Had the horizon been water, the nucleus would have come in sight at 2.19, as it was, an interval of 11m. was required to bring it above undulations of the earth. We thought best not to telegraph before seeing the nucleus, but as soon as we positively knew the apparition to be a comet, haste was made to send dispatch. The village is on a branch railroad and telegraph offices are not open nights, so we had to send to the residence of the operator, arouse and engage him to go to the office and send telegram. This took time, and it was not until after 3 A. M. that message was sent. Meanwhile we endeavored to place telescope on nucleus but were unable to, as there was a tree in range, causing another delay until 3.30, when observation was made—the nucleus being an hour above horizon and in apparent

R. A. - - - - - - -	5h. 34m,
δ - - - - - - -	43° 10′

a rough position, as no corrections were made for refraction or parallax.

The telegram read: "Vast comet in northeastern heavens." After mature consideration we regret using so many words, one only—"Comet,"—was all that was necessary, when the acute observer, Swift, would have been on the alert at once. Before sunrise we were favored with 30 minutes good definition, when two envelopes were seen, the nucleus extending a bridge to the external surface of the inner one. Since, the nucleus has changed form, is no longer round, but has prolonged into a beak-shaped mass, and looks like Comet III., 1862, August 29, as drawn by Challis (Chambers' Astronomy, p. 322). The cometary matter is of great tenuity, as it was seen to run over a sixth magnitude star at 10h. June 28, which passed about 15″ from nucleus, yet it was visible through the immense volume of gas.

The comet was seen from many points in the Western States twenty-four hours before noticed at this place, by steamboat hands, street-car drivers, railroad conductors, night-watchmen, policemen and many others whose business required them to be out all night.

NEW WINDSOR, ILL., *July 1st*, 1881. EDGAR L. LARKIN.

SCIENCE

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CONTENTS.

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